DISCUSSION BEFORE THE WIRELESS SECTION, 4TH NOVEMBER, 1942

Sir Stanley Angwin: This paper is an outstanding example of the successful scientific analysis of a series of observations obtained under practical conditions, and leading to some theory which can be well applied in the interests of design. I am not sure that it is obvious from the paper what a tremendous amount of statistical analysis was necessary in order to arrive at any conclusion at all, let alone the conclusions which have in fact been reached in this paper. At the rate at which the recording tape was run, about 6 ft. of tape was used in 24 hours, so that over the whole period during which these observations were being carried out a length of about 1½ miles of tape was used.

It is unfortunate that it is no longer possible to continue the observations on the radio link to the Channel Islands, but we hope that either there or elsewhere some opportunity will be found in the future to continue observations of this nature, in order to fill in some of the blanks which must be filled in before any conclusive deductions can be drawn from the observations already made. This is a very striking example of the desirability of not drawing too hasty conclusions from samples which are not sufficiently diverse or which have not been taken over a sufficiently long period of time to enable a theoretical interpretation of the results to be made with certainty.

Turning to the results of this analysis, there is an indication, first, that fading increases with a decrease in wavelength over the distances with which we are concerned in this paper; and, secondly, that the fading is both more rapid and of greater depth on the shorter wavelengths. Having those two conclusions in mind, the further one might perhaps be drawn that over distances which are appreciably beyond optical range, and under the conditions which are analysed here, wavelengths at any rate much shorter than 5 m. (the shorter of the two under consideration in the paper) are unlikely to be as effective in the present state of the art.

An experiment, not referred to in the paper, was carried out in order to ascertain whether there were changes of polarization which might account to some extent for the fading phenomena. A 5m. vertical array was erected at Chaldon which had an effective discrimination over reception for horizontally-polarized rays of the order of 50 to 60 db., and observations made simultaneously on the vertical ray and on the horizontal ray showed that during periods of deep fading on the horizontally-received wave there was no appreciable gain on the vertically-polarized reception. That to some extent was corroboration of the suggestion that the fading cannot be due to reflection from the ionosphere.

From the examination of these curves indicating very deep fading, slow fading and (in some cases) persistent fading, I should not like the impression to be gained that this was a particularly poor circuit from the point of view of transmitting speech. Good commercial speech was in fact normally attained on this circuit using automatic gain control. The latter proved of great value under the worst conditions, where a variation of field of the order of 4·5 db. meant a variation of output from the receiver of the order of 1 db. Even in the conditions shown in the paper, there were very few occasions when the circuit was uncommercial from the point of view of putting it in the telephone network as a link in the circuit from Guernsey on to the inland telephone system. The limiting factor in most cases still remained the effective noise-level rather than the disturbances due to fading.

The authors had to choose a value for defining what would be classified as a fade for the purposes of their statistical analysis, and presumably the figure of 15 db. upon which they decided is a reasonable one to take. It would be interesting to know what determined the authors' choice in this respect, and whether they feel that the analysis would have been disturbed if some other value had been chosen.

Mr. E. C. S. Megaw: It is very interesting, and rather surprising, that the authors find no sign of diurnal periodicity in their records. Have they any comments to make in that connection on the observations of Hull, who found a definite diurnal periodicity in some cases of propagation of similar wavelengths?

In introducing the paper Dr. Smith-Rose suggested that, as large signal variations had been observed, it was very likely that multi-path transmission was involved. But when he showed, later, in a slide taken from an earlier paper, the comparison of the total range of variation in the observed results with the calculated field strengths expected first for diffraction only over an earth without any atmosphere, and then the two higher curves corresponding to the added effect of refraction in a dry and a damp atmosphere, the agreement between the range of variation observed and the highest and lowest of the three curves was remarkably good. It seems, therefore, that, taking only that part of the evidence, one might be tempted to say that the whole variation could be explained in terms of single-path transmission, and that the variations are simply due to different amounts of curvature of the rays.

The authors' results show, apparently, a great difference between 5 m. and 8 m. as regards the overall variability of the signal. Do the authors think that this is typical of the variation of propagation characteristics with frequency, and that if one went on to a higher frequency that variation would increase at a similar rate; or do they think that possibly it might be related to purely accidental circumstances in this particular link? For instance, supposing the interpretation of the results which I outlined just now was wrong, and we had to assume these variations to be caused by multi-path transmissions, one wavelength might have been unlucky in that the receiving position happened to be near a minimum in an interference field for a large part of the time, while this was not true for the other wavelength.

There are two minor points which I should like to mention in conclusion. First, I think that the value of Fig. 1 would be increased if an indication were given of the scale of the signal variations, or at least if we were told whether the scale is approximately a linear one or approximately an exponential one. My second point is: What do we in general mean by "optical range?" I suggest as a possible answer that we should use, where it is relevant, the term "geometrical optical range" instead of simply "optical range," and that by that we should mean the optical range as defined by the usual geometry over a curved earth, using the real radius of curvature of the earth.

Mr. R. Naismith: In the calculation of maximum usable frequencies it was formerly the practice to consider that the tangential ray would be so highly absorbed that it would have insufficient energy left to reach great distances. The ray which leaves the earth at an angle of from 2° to 3° was assumed to provide the greatest practical range. If, however, it is safe to assume even 1° or 2° of bending in the lower atmosphere, the maximum usable frequency, and the maximum distance of transmission with only one reflection in the ionosphere, will be increased and the theoretical computation will be brought more nearly into line with the practical observation. The frequency band involved will lie within the range 30–50 Mc./s., which overlaps that discussed in the paper.

It is satisfactory to note that the authors find no evidence of highly reflecting regions in the lower atmosphere.

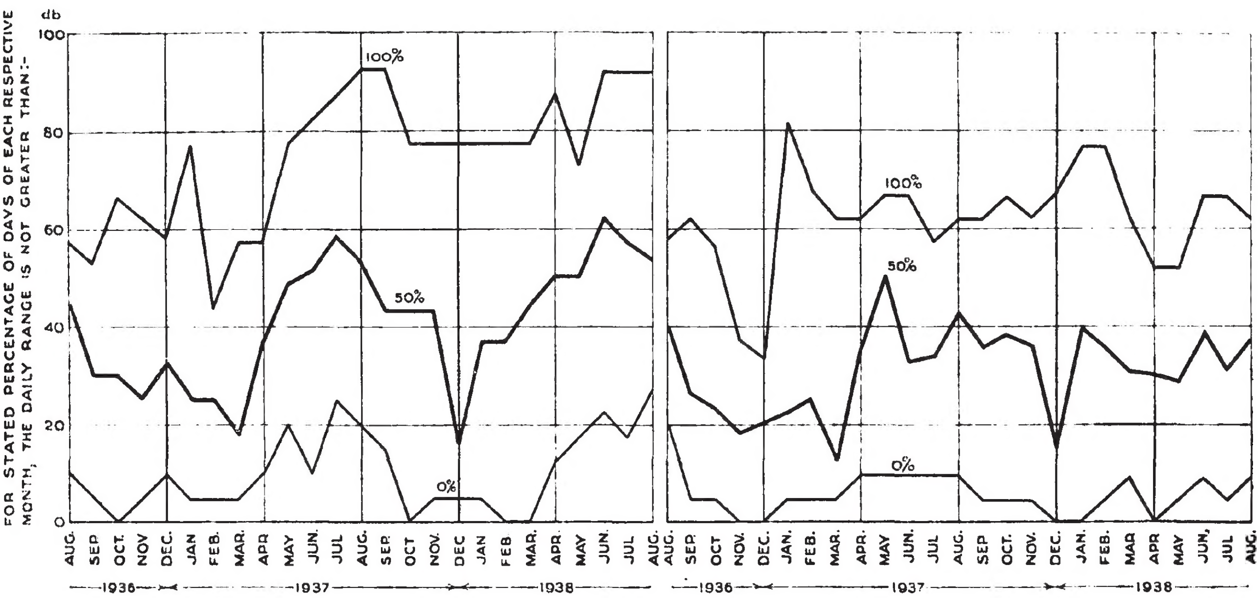
Mr. A. H. Mumford: With the object of providing an ultra-short-wave radio-telephone link between Guernsey and England, experiments were made between Guernsey and Dartmouth in 1934, wavelengths of 3 m., 5 m. and 8 m. being selected for the preliminary transmissions; the 3 m. tests were soon abandoned since they gave very poor results compared with the 5m. and 8m. transmissions.

Dartmouth was selected mainly because it gave the shortest possible radio path (some 78 miles long), but it had the disadvantage of requiring lengthy land-line connections to London. Consequently a further series of tests was commenced between Guernsey and Shaftesbury; that radio path was 105 miles long, of which some 20 miles were over land. Although the initial experiments there were very promising, the results over a period of a month or two were not nearly so consistent or satisfactory as in the Dartmouth link, owing (it was thought) to the inclusion of so much land in the transmission path. The decision was then made to confine the transmission entirely to an oversea path, a site at Chaldon being selected in preference to Dartmouth as it gave the shorter land-line route. The Guernsey–Chaldon radio links went into commercial operation as part of the telephone network of this country in May, 1936.

A vital point in relation to links which have to be connected into the telephone network, and particularly such a relatively short link as the one discussed by the authors, is that 24-hour communication must be guaranteed. This was one of the factors responsible for the decision to continue transmission simultaneously on both 5 m. and 8 m. rather than to locate both transmissions in the one waveband; thus minimizing the effect of any long-term variations which might subsequently occur.

In Section (2) of the paper the statement is made that "The records actually give a measurement of the current in the first detector stage of the receiver." Actually it is not the first detector but the first intermediate-frequency amplifier valve anode current which provides the record. The original records were made on two separate recorders, but at a later stage a single recorder having two pens and a common time base was employed. This was of particular value in demonstrating the lack of simultaneous conditions of field-strengths. The double recorder was in use from March, 1938, onwards.

There has been a good deal of discussion as to the extent of the fading, and Fig. A shows the daily range of the received field as being not greater than the value indicated for the stated percentage of days of each month over the period August, 1936, to December, 1938, (*a*) for the 5m. and (*b*) for the 8m. transmission. These figures give an idea of the wide range of field strengths with which the communication engineer has to deal in his equipment.



(*a*)

(*b*)

Fig. A

(*a*) Results obtained on 5m. transmission.

(*b*) Results obtained on 8m. transmission.

The data which records of this type summarize are of particular value to the engineer who is continually faced with the problem of forecasting from knowledge and experience the type of service any proposed radio link is likely to yield. Even over optical paths, where steady conditions can be expected, the problem is by no means simple; and it is our practice to carry out site tests in every case as far

as is possible. When, however, non-optical path links are involved the problem assumes further difficulties and becomes more indeterminate. It is evident from the data given in this paper that one would have to carry out site tests extending over at least 12 months before one could feel any confidence in forecasting what results would be realized by an actual installation. Anything which adds to the knowledge available to the communication engineer, and thereby enables him to anticipate with some reasonable degree of accuracy the field condition he is likely to meet in actual practice, is therefore of the greatest value.

Prof. P. A. Sheppard: During the last 20 years or so, the radio engineer has developed a probe leading to valuable information as to the structure of the ionosphere; this paper suggests that he is now about to provide the meteorologist with a slightly different probe, wherewith to explore the structure of the lower atmosphere, or troposphere. Colwell and his co-workers in the U.S.A. claim, I believe, to have advanced some distance along that path, and to be able to demonstrate the existence of atmospheric discontinuities either of temperature or of water vapour, some of which characterize the boundaries between the large air masses, interaction between which gives rise to our weather. I should be interested to know what the authors of the present paper feel about that work.

Hull, transmitting over land, has observed diurnal variation, whereas the present authors show that diurnal variation did not occur for the path concerned in their case, which was solely over the sea. These results are not unexpected. The vertical gradients of temperature and water-vapour in the lowest thousand feet or so of the atmosphere over the land are frequently reversed as between day and night; and the authors of this paper point out that such variations give rise to variations in transmission conditions. Over the sea on the other hand diurnal variation of temperature and humidity is almost entirely absent. Similar remarks will not, however, apply to atmospheric discontinuities at higher levels, of the order of a few thousand feet, which normally characterize anticyclonic conditions, and which do markedly affect transmission conditions. These temperature and humidity discontinuities are characteristic of the air, whether over the sea or the land.

Mr. W. L. McPherson: I should like to associate myself with the request for a clarification of what is meant by "optical range." Does this term mean the geometrical range, or the range including refraction effects?

It has been said that fading is normally due to interference between two paths. Assuming that the fading discussed in this paper is due to that cause, we should expect it normally to be greater the shorter the wavelength, for then a given change in path conditions would cause a greater change in the fading on the two waves. This assumption is rather borne out by the fact that the fading on 5 m. is worse than that on 8 m. On the same assumption we should expect that fading would be much more pronounced on 17 cm., and it should be possible to get some check on the theory by comparing 17 cm. propagation with propagation on wavelengths of 5–7 m. For the only data that I have relating to 17 cm. propagation the comparison is not quite valid, because in the case of the Guernsey link the path is partly within optical range and partly beyond it, whereas in another case to which I shall refer the path was wholly optical on either definition.

In this latter case we could not associate fading with any particular atmospheric conditions. Our analysis, extending over a period of 9 months, showed that fading occurred not under any stable atmospheric condition but when the conditions themselves were changing, preferably suddenly. We found no difference between day and night conditions, no difference between heat and cold, dry or wet or snow, or between clear atmosphere and fog; but there was one occasion on which the temperature-inversion layer showed up clearly, and that was when a big fog bank drifted over the Channel area and began to settle, and as it settled the signal strength increased above normal until it was surprisingly good. The signals then faded out completely, but after 20 min. came back slowly; and 2 hours later, when the fog settled heavily at both ends of the link, the signal strength was normal. That was undoubtedly a case of interference by reflection from a fog bank.

Mr. G. Millington: The authors attribute the more rapid fading on 5 m. to the shorter wavelength, and not to the horizontal polarization as compared with the vertical polarization used on 8 m. If experiments have been carried out to show that the type of polarization is immaterial, I feel that this should have been emphasized in the paper, as I believe that certain American workers did find that fading tends to be more rapid with horizontal than with vertical polarization.

With regard to the question whether the increase of range in the diffraction region is due to a gradient of refractive index or to actual tropospheric reflections, Beverage published some years ago in America curves for three circuits in the 1m. to 10m. wave-band, which gave a big scatter of points. The large majority of these lay between two well-defined envelopes, the lower corresponding to unaided diffraction and the upper to the effect of a gradient bending the ray so that its radius of curvature was about twice that of the earth.

There is also definite evidence of tropospheric effects. I believe that the television signals from Alexandra Palace were sometimes received in Scotland, and this could hardly be explained by refraction alone. Such signals are usually very erratic, and the explanation is almost certainly tropospheric reflections which may only last for a few moments. These reflections can be regarded, as in the paper, as a refraction in the troposphere with a very small radius of curvature, or alternatively as a partial reflection from an actual discontinuity of refractive index. With a discontinuity as small as 10^{-5}, total internal reflection can occur if the incidence is nearly glancing, say within $0\cdot25°$; and, even though the reflection coefficient falls off rapidly as the angle increases, the relatively weak tropospheric wave can become dominant over the diffracted wave beyond the optical range.

Dr. E. Billig: As the question of optical range has been mentioned several times in the discussion, I would suggest the following definition: The transmitter and receiver are said to be within optical range if the straight line passing through them does not touch the surface of the earth anywhere. The total optical range is thus made up of the range of the transmitter and the range of the receiver, each given by the distance to its apparent horizon.

Would it be right to suggest that the superior quality of

transmission over sea is due to the much more constant value of humidity prevailing over sea than over land? The diurnal variation in the strength of signals received over land could then probably be accounted for by the regular formation of dew.

Mr. W. E. Willshaw: I wish to make one comment on the secular variation of fading, dealt with in Fig. 3. Looking casually at the graph shown in that Figure one gets a false impression of the difference between the amounts of fading for the two wavelengths, in that the zero is suppressed and the range of time given in the vertical scale is only about one-half of the total range from zero. Do the authors think that the differences shown in this graph are in some way due to reflections from the layers of the upper atmosphere? There does not appear to be any obvious reason for such differences when transmission is regarded as a function of diffraction and refraction around the surface of the earth.

Mr. H. L. Kirke: It would have been of considerable assistance to the reader if a vertical scale had been included in Fig. 1.* Also, it would have been an advantage if the pictures had been made somewhat larger.

A point which I notice in a large number of these diagrams is that the fading is frequently of a type in which the signal strength decreases considerably. It is not the type of fading to which we are accustomed, where the signal strength varies above and below a mean level.

In connection with fading and the diurnal effect, it may be of interest to quote some measurements made on the Alexandra Palace transmissions. The first series of measurements was made from March to May, 1937, and a series of observations was taken at 25-mile intervals on the Great North Road from Baldock to John o' Groats, radiating on 41·5 Mc./s. At Stamford, a distance of 73 miles, the daylight field had a mean value of 30 mV/m., fading slowly about $\pm$ 3 db. The mean night field was 20 mV/m. with a fade of $\pm$ 3 db. Thereafter the daylight field was soon lost, all that was received being short intermittent bursts of radiation. This condition obtained all the way to John o' Groats, the peak intensity of the bursts gradually growing less. However, it was observed that in most cases the day peak field exceeded the night peak field by about 6 db. or more. Observations on the return journey confirmed this effect.

In April, 1939, a series of measurements on the same transmitter was carried out at Birmingham and later at Droitwich. In both places it was found that the daylight field exceeded the night field by some 4 to 8 db. In the case of Droitwich the fading at ground-level was considerable, both by day and by night, but at a height of 100 feet decreased very considerably. In the case of the Birmingham measurements, this effect was not noticeable as the measurements were done at the University tower and it was found that the field at the bottom of the tower was as high as, if not greater than, at the top of the tower. Presumably the tower was acting as some sort of feeder transmitting the energy received at the top to the earth.

In June, 1939, measurements were made at Sale, Manchester, and on two consecutive days a day field of 8 mV/m. and a night field of 4 mV/m. was obtained. There seems also to be general evidence, from conversations with people who have listened to or looked at the Alexandra Palace transmissions at a distance, that the daylight field is greater than the night field.

* This has been done for the *Journal*.

Dr. J. S. McPetrie (*communicated*): Having seen some of the records analysed by the authors, I must congratulate them on their having been able to arrive at such definite conclusions about the effect of the atmosphere on radio propagation. The result given in Fig. 2, showing the relation between received signal and the atmospheric conditions, is particularly convincing. The records, however, were obtained on a commercial link and the information is consequently rather limited. For example, vertical polarization was used on the 8m. link and horizontal polarization on the 5 m. It is dangerous, therefore, to draw any hard-and-fast conclusions from the observed differences between the results obtained on the two wavelengths as they may be due to the change in wavelength, the change in polarization or a combination of these factors.

In the theoretical section of the paper the authors go to some pains to explain how complete reflection may occur at layers in the atmosphere at which the dielectric constant of the atmosphere undergoes a sudden change. This complete reflection occurs, no doubt, in the presence of temperature inversion. Such complete reflection, however, is not required for the production at the receiver of a relatively strong field due to reflection at a height of a kilometre or so above ground. If the dielectric constant changes by as small an amount as 10^{-5} the amount of reflection is sufficient for the reflected wave to give a field strength at a receiver position beyond optical range of the transmitter several times greater than that due to any waves propagated at lower heights in the atmosphere.

Dr. R. L. Smith-Rose and **Miss A. C. Stickland** (*in reply*): Dealing first with points raised as to the actual presentation of the paper, a scale has now been added to Fig. 1, which makes this diagram self-explanatory. The criterion for fading was taken as a variation from the mean value for the day equal to one-quarter of the distance between two of the horizontal lines, i.e. 0·25 mA, and this corresponded to a field-strength variation of 15 db. As stated in the paper, other methods of determining fading were investigated but were found to give similar results; these included taking a limiting criterion of 0·5 mA in one case, and, in another, including in the 0·25 mA limit only stretches of fading lasting two minutes or longer.

Various contributors have raised the question of definition of the term "optical range": throughout the paper this is taken as geometrical optical range which is given by the expression $\sqrt{(2Rh)}$ for a transmitter or receiver at height h, where R is the earth's radius, all lengths being measured in the same units.

With regard to the different effects of horizontal and vertical polarization, Sir Stanley Angwin has described some experiments carried out by the Post Office which suggested no appreciable advantage of one polarization over the other, while in the work studied in the paper it has been impossible to distinguish between the effects of horizontal and vertical polarization on the one hand and shorter or longer wavelength on the other, since the 5-m. observations were all on horizontal polarization and the 8 m. on vertical polarization. The same ambiguity exists in the American work referred to by Mr. Millington, since these workers[3] state that the horizontal component exhibited the worse fading and that fading was in general

greater on the shorter wavelength. These statements also answer in part Mr. Megaw's question as to generalization of the effect of wavelength on fading; at present data are not sufficient for generalization.

In answer to the queries of Mr. Megaw and Mr. Kirke as to the presence or absence of diurnal variation, the evidence so far available would seem to indicate that this depends upon whether propagation is over land or over sea; thus Hull's observations and those based on the Alexandra Palace transmissions were both over land paths, in which cases a diurnal variation might be expected owing to changes in meteorological conditions. We are very grateful to Prof. Sheppard for his remarks on the meteorological aspects of this question which confirm our deductions and enable us to base these statements on a much firmer foundation.

The question of single- or multiple-path transmission is still an open one, which cannot be decided until much more experimental material is available; both the aspects of discontinuities mentioned by Mr. Millington and Dr. McPetrie and of variations in refractive bending suggested by Mr. Megaw will then be of use to determine the matter more exactly.

We do not think that the secular variation of fading discovered in the course of the analysis can be attributed definitely to any certain cause as Mr. Willshaw suggests, since the amount of data is not yet sufficient. The obvious thing to correlate for ionospheric effects is perhaps the variation of sunspot numbers; this was done, but with no helpful results.

With regard to Prof. Sheppard's query as to our opinion of the work of Colwell and his co-workers on the use of ultra-short waves as a meteorological probe, we are reluctant to make a definite statement as yet. There are perhaps reasons to expect some good correlations, but we feel that a much wider range of observations and a clearer statement of method, wavelength used, etc., are necessary before one can pass judgment.